

ASTRONOMICAL

OBSERVATIONS

MADE AT THE

ISLAND OF ST. HELENA,

By the Rev. NEVIL MASKELYNE, A. M.

Fellow of TRINITY-COLLEGE, in the University of CAMBRIDGE, F. R. S.

Read at the ROYAL SOCIETY,
Dec. 20, 1764, and Jan. 24, 1765.

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TO
THE RIGHT HONOURABLE
The Earl of MORTON, PRESIDENT,

AND
The FELLOWS of the ROYAL SOCIETY,

THE
Following Observations, made when I was employed,
by their Appointment, at ST. HELENA,

ARE
Most respectfully presented, by,

His Lordship's,

And the Royal Society's,

Most obedient,

Humble Servant,

Nevil Maskelyne.

1787

THE RIGHT HONOURABLE

The Earl of MONTGOMERY, President,

AND

The Fellow: of the Royal Society,

THE

Following Observations made when I was employed
by the Admiralty, at ST. HELENA.



Presented by

His Lordship,

And the Royal Society,

Most obediently

Presented by the Honble Secretary,

Nevil Maskelyne.

*Extract of a Letter from Mr. John Horsley,
Fourth Mate on board the Glatton East-
India Ship, to the Rev. Mr. Nevil
Maskelyne, F. R. S. dated Batavia, Nov.
16, 1763, giving an Account of his
Observations, at Sea, for finding out the
Longitude by the Moon.*

Dear Sir;

YOU was so good as to express a desire of
hearing from me, by every opportunity,
during the time of my voyage; a request that I
shall always comply with, with a greatdeal of
pleasure.

I have the misfortune to inform you of our having
lost our passage to China, the original occasion of
which was our late departure from England. We

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arrived:

arrived at Bencoolen July 29, where we were detained till the 13th of September. We found the winds strong set against us in the China seas, which obliged us to bear away for this place.

I shall now give you an account of the observations I have made, since I have been out, and the success attending them.

We sailed from Spithead March the 8th, 1763; the 19th I had four observations of the distance of the Moon from the Sun; by taking the medium the longitude agreed exactly with that by account. The 21st I had another observation, and, the same day, saw the island of Madeira, the body of which, according to this and the former observations (they agreeing exactly) I made to lie $17^{\circ} 18'$ west of London, which differs only 18 minutes from what it is laid down in the chart. The success I met with in this first attempt gave me great satisfaction, and made me continue my observations regularly to the island of St. Paul's, which we made July 5. The day before I had three observations of the distance of the Moon from the Sun. July the 5th, the body of the island bearing by the azimuth compass S. 27° W. distance six leagues, the sky remarkably clear and fine, and the ship having hardly any motion, circumstances all in my favour, I took nine observations of the distance of the Moon from the Sun, the captain and chief mate assisting me in taking the altitudes. I divided them into three sets, and worked from the medium of every three; by which I made the longitude of the ship as follows, $75^{\circ} 15'$, $75^{\circ} 25'$, $74^{\circ} 40'$. The three observations, I took the day before, made the longitude of the ship $74^{\circ} 38'$ and $73^{\circ} 32'$ which brought

brought forward to the noon of July the 5th made $75^{\circ} 45'$ and $74^{\circ} 39'$. Taking the medium of the whole five setts I made the longitude of the ship at noon $75^{\circ} 8' 48''$ east of London. Subtracting from thence the difference of longitude, the bearings and distance of the island gave $= 8' 37''$ west, I made the longitude of St. Paul's $75^{\circ} 0' 11''$ east of London, and $58^{\circ} 0' 11''$ from the Cape of Good Hope. By my account kept from an observation taken June the 18th, I made it $73^{\circ} 35'$ east of London, and $56^{\circ} 35'$ from the Cape, which differs $1^{\circ} 25'$ from what I make the true longitude: most of the accounts on board were between two and three degrees to the westward of mine. The longitude of this island having never been determined by any other method than the runs of ships to the Cape, there are hardly any two charts or books that lay it down alike, they differing from 71° to 74° in their accounts, which made me put little dependance upon any of them.

On our arrival at Bencoolen I took three observations of the distance of the Moon from the Sun, in the road, by which I made Fort Marlborough to lie in $103^{\circ} 50' 45''$ east of London.

I was on shore five or six days in hopes of getting some observations of Jupiter's Satellites, but was disappointed by the cloudiness of the nights; so that I got nothing for my pains but a fever, which had nigh cost me my life, terminating at last in an intermitting one, which has continued with me ever since, neither does it seem to have any inclination to leave me at present.

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I have saved all the observations I have made, and the work of them, which I should have sent you a complete copy of, if I had been well enough to have transcribed them.

I am,

Yours, &c.

John Horsley.

“ Mr. Horsley, whose skill and diligence are better evinced by his own account than by any encomiums I can give them, made use of a quadrant made by Mr. Bird, and my British Mariner’s Guide, for determining the longitude of the ship at sea.

N. Maskelyne.

THE following observations were taken with a reflecting telescope, of two feet focal length, made by Mr. Short (of a similar size and construction to those used in the observation of the Transit of Venus, by himself at Saville House, by Mr. Green at Greenwich, and by Mess. Mason and Dixon at the Cape of Good Hope), with an equal altitude instrument made by Mr. Bird, and a clock, with a gridiron pendulum, made by Mr. Shelton, an account of whose going, at Greenwich, before my departure for St. Helena, and immediately upon my arrival there, is contained in Phil. Trans. Vol. LII. Part II. Page 434. and the difference of gravity between those two places thence deduced.

The almost continual cloudiness of the skies, at the Island of St. Helena, renders it a very inconvenient place for the making of astronomical observations, which I had the mortification to experience in losing the sight of the exit of the planet Venus, from the sun's disc, on the 6th of June 1761, to observe which was the primary motive of my going thither. I should have thought myself, in a great measure, compensated for this mischance, if I had been enabled, by the help of the ten foot sector, provided me at the expence of the Royal Society, either to prove or disprove the existence of a sensible annual parallax of the star Sirius, some reasons for the probability of which I laid before the Royal Society, in a paper since published in Phil. Trans. Vol. LI. P. II. p. 889, and, at the

the same time, offered a proposal for the discovery of the same, by observations of the zenith distance of that star, to be made at the island of St. Helena. But, unfortunately, when I came to set up the sector there (which, through the tardiness of the workman in finishing it, I had not had an opportunity of proving, as I had wished, before my departure from England) I soon found a strange irregularity in the observed zenith distances of the stars, amounting to 10, 20, and sometimes even 30 seconds. After having satisfied myself, by various trials, that these great differences in the observations did not arise from any bending of the tube of the telescope, which constitutes the radius of the instrument, or from any looseness in the object-glass, or instability of the wooden three-legged stand, which supports the sector, I, at last, found the cause of error to lie, where I had least suspected it, in the imperfection of the suspension of the plumb-line (which is a fine silver wire) from the neck of the central pin; for, upon taking the loop of the plumb-line off the pin, and putting on again, after turning it half round, or putting on a new one, I found the plumb-line would apply itself to a different part of the limb of the sector, commonly by 10, and frequently by 20 seconds. This experiment, with the same event, I had the honour of exhibiting before a committee of the Royal Society, for their satisfaction, as to the cause of the failure of my intended observations, September 11, 1762, at the British Museum.

The irregularities in question evidently arose from the friction of the loop of the plumb-line against the neck of the central pin; a fault, to which most of the

the sectors, made before mine, have probably been liable. Indeed the fault became more glaring here, by the workman's having made the diameter of the neck of the central pin so large as $\frac{1}{10}$ th of an inch; but that the errors cannot be entirely removed by lessening the neck of the pin, I can assert from my own experience, having caused a pin to be made with the neck only $\frac{1}{70}$ th of an inch in diameter (and beyond that it cannot well be reduced) by which I still found an irregularity in the suspension of the plumb-line, to the amount of 3'', a quantity, though seemingly small, yet of great consequence in the nice observations to which this instrument is generally applied, and which it is capable of taking to a prodigious exactness, when the suspension of the plumb-line is accurately provided for. Mr. Bird has contrived one of six foot length, for settling the limits between Pennsylvania and Maryland, in which the plumb-line is adjusted so as to pass over against, and bisect a small point at the centre of the instrument.

I cannot, on this occasion, omit remarking that the late learned Abbé de la Caille's sector, with which he made his principal observations, from some of which I inferred the probability of an annual parallax of Sirius, seems to have had a like fault with my sector, as may be inferred not only from the differences in the observations themselves, but also from the brief account of the suspension of that instrument, contained in a letter with which I have been favoured by M. Delalande from Paris, an extract of which I presented to the Royal Society. Vide Phil. Trans. Vol. LII. Part 2. Page 607.

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Let me observe also, that the 9 foot sector, made in London by Mr. Graham, with which the gentlemen of the Royal Academy of Sciences at Paris measured a degree of the meridian, at the polar circle, and afterwards a degree between Paris and Amiens, had, at the time of their making those observations with it, the suspension of the plumb-line contrived after the same manner as mine (whatever alterations may have since been made in it) as appears from M. De Maupertuis's minute and accurate description of the said instrument, in his account of the measure of the degree of the meridian between Paris and Amiens: for he there says, that the part of the central pin, on which the loop of the plumb-line was hung, resembled the meeting of two opposite cones at their points; which is an exact description of the form of the neck of the central pin of my sector. But, though this capital defect in the suspension of the plumb-line of my instrument (which I could not correct, at St. Helena, for want of workmen and tools) prevented me from deciding the question concerning the annual parallax of Sirius; yet, as I am conscious, the want of success did not arise from any fault of mine, I shall endeavour to console myself for my disappointment, by the reflexion, that I may, at least, have contributed something to the benefit of astronomy, by having discovered, by my experiments, the imperfection of the above-mentioned method of suspension of the plumb-line in sectors, which no one ever suspected before, and so may be the means of preventing any more instruments of this kind being constructed in the like faulty

faulty manner, and consequently any future astronomers being deceived in their observations.

There still remained one object worthy of attention, which I had also proposed to the Royal Society, and received their encouragement to proceed in it. This was the observation of the horary parallaxes of the moon, by the difference of right ascension in time between the moon's enlightened limb, and stars near her parallel of declination: a kind of observation never before made to my knowledge, by any astronomer, in a latitude so near to the equator, as St. Helena, which, by determining the mean horizontal parallax in that latitude, infers also, by a proportion, which will come out sensibly the same upon any probable hypothesis of the figure of the earth, the mean equatorial parallax, which hath never yet been deduced in any manner so nearly direct.

For the purpose of making these observations, I was provided with a polar axis, suitable to the latitude of the place, on which my reflecting telescope was mounted, and a particular additional eye-piece, having fine silver wires stretched in the focus of the nearest eye-glass. The cell containing the wires being moveable round about, by means of a screw, it was easy to cause any star near the moon's parallel of declination to run exactly along one of the wires, which may be called the directing wire, from the centre to the extremity of the field of the telescope. The exact instants of the stars passing three wires placed perpendicular to the former, which may be called the horary wires, representing small portions of horary circles, were noted by the clock

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to the exactness of $\frac{1}{4}$ th of a second of time; as were also the instants of the moon's enlightened limb passing the same wires. It is manifest that the difference of time, observed by the clock, between the star and the moon's limb passing the horary wires, reduced to sidereal time, and from thence into parts of the equator, is the apparent difference of right ascension between the star and the moon's limb passing the horary wires. The same observations repeated after an interval of some hours gave the present difference of right ascension between the star and the moon's limb; whence the moon's apparent motion in right ascension, or the difference of these differences is known; which subtracted from the moon's motion in right ascension, in the given interval of time, owing to her proper motion in her orbit, computed, in the most exact manner, from the best tables, leaves the remainder for the change of the moon's parallax in right ascension between the two times of observation; the ratio of which to the horizontal parallax at that time being also computed, the horizontal parallax of the moon is known: and consequently, by the help of a proportion borrowed from the tables, the mean horizontal parallax of the moon in the latitude where the observations are made. The mean horizontal parallax being deduced in this manner from a great many observations on different nights, the mean of all the results may be taken, as approaching very near to the truth: for the advantage is so great from taking a mean of a great number of astronomical observations, that any degree of exactness required, may be

be thereby obtained, provided they are not liable to any constant and uniform cause of error: as has been clearly shewn by my late worthy and learned friend Mr. Thomas Simpson, Phil. Trans. Vol. XLIX. Part I. Page 82; and also in his Miscellaneous Tract, Page 64. Therefore I cannot but think, that, from a considerable number of such observations, the mean horizontal parallax, and thence the mean equatorial parallax of the moon might be deduced certainly to a single second, or ever nearer if required.

In hopes of attaining such a degree of exactness, I endeavoured to multiply my observations as much as possible: yet, through the great cloudiness of the island, could not obtain more than three nights complete observations. If these should appear too few to attain the exactness proposed, yet they may contribute, in a good measure, thereto. However, I have since had an opportunity, during my residence at the island of Barbadoes, in the latitude of 13° north, which 3° degrees nearer the line than St. Helena, to repeat these observations to a very great number, from which, I doubt not, the mean equatorial parallax of the moon may be accurately determined.

I shall here desire to remark, that, if the like observations were repeated in different latitudes, they would probably afford the best means yet proposed for ascertaining the true figure of the earth; as they would determine the ratio of the diameters of the parallels of latitude to each other, the horary parallaxes being proportional thereto: and, after all the experiments and observations that have been made

on this subject, we shall probably, at last, be indebted to observations of the moon's parallax for the best determination of it: for though the earth affords but a small base at the moon, yet, by repeating these trials, and comparing their results, we may hope to attain that degree of exactness, which we could never expect from fewer observations.

As I look upon the species of observations here spoken of to be of very important use for the improvement of astronomy and geography, and as such desire to recommend the practice of them, especially to those who may have occasion to visit countries of distant latitudes; I shall briefly mention such further particulars, which the experience I have had, and my attention have suggested to me, the observance of which may conduce to the greater accuracy, as well of the observations, as of the consequences to be deduced from them.

I apprehend the use of a polar axis to be very necessary for rightly managing the telescope, as well for finding what star it is proper to compare the moon with, as for preserving the position of the wires unvaried, after their adjustment. A very nice and exact polar axis is not requisite; but a cheap one, and such an one as may easily be provided, will suffice. Mine was formed by a brass socket, making an angle with the horizontal top of the stand equal to 16° or the latitude of the place, receiving the brass cylindrical support of the telescope, instead of the perpendicular socket of the common stand: and the telescope was firmly confined in the socket by a pointed screw which passed through one side of the

the socket into some of the holes, which were drilled in the support of the telescope.

The polar axis may be set near enough to the direction of the meridian, by a magnetic needle, allowing for the variation; or, even by the sight, provided the walls of the observatory be built nearly north and south. This being done, and the directing wire being brought into such a position, that the star may run exactly along it from the centre to the extremity of the field of the telescope; then if the screws of the rack work be turned, and the star be brought back to the intersection of the wires, it will be found to run exactly along the directing wire again; and this I generally found would be the case, even for a very considerable space of time, though the star had, in the mean time, advanced a considerable way from east to west by the diurnal rotation; so that it is not always necessary to re-adjust the wires after each set of observations, though it may be proper to examine whether they require it or not. Hence it follows, that there can be no danger of disturbing the position of the wires after their adjustment, by bringing the star back to the entrance of the telescope, in order to observe its passage across all the horary wires.

Sometimes it so happens that a proper star cannot be found that precedes the moon, to compare her with; in such a case, the observer must compare her with a star following her, and adjust the wires by making some bright point of the moon run along the directing wire, which is a more exact method than by making the directing wire a tangent to the moon's north

north or south limb. Here, indeed, the directing wire cannot represent a parallel to the equator, on account of the moon's continual change of declination, but will make a small angle therewith; which may be computed, and the observations corrected accordingly. But the correction may be easier made, as follows, let a express the moon's apparent angular motion about the pole of the world in four minutes of time, being the difference of her proper motion in right ascension, and the change of her parallax in right ascension: d her apparent motion and declination in the same time, b the difference of the apparent declination of the moon, and that of the star, r the radius, and c the cosine of the moon or star's declination; the correction to be applied to the moon's right ascension, or the difference of right ascension of the moon and star is $b \times \frac{d}{a} \times \frac{r^2}{c^2}$. If the moon is approaching the star's parallel of declination, she will come to the horary wire relatively too late for the star, and her right ascension, deduced immediately from that of the star, will be too great, and must be diminished by the correction here mentioned; but, if the moon is receding from the star's parallel of declination, she will come to the horary wire relatively too soon for the star, and her right ascension, immediately deduced from that of the star, will be too small, and must be increased by the above-mentioned correction.

There is another attention, which the nice observer will not think too trifling for his notice, namely, to examine whether the wires of his telescope are

are placed at exact right angles to each other (which they seldom are) and, if they are not, what the quantity of deviation is, in order to make an allowance for it in the reduction of the observations. This may be tried several ways. I examined the wires of my telescope at Barbados, by a great many observations of the difference of right ascension of stars, which differed considerably from each other in declination, namely Arcturus, and the little star accompanying it, and the virgin's spike, and a small star preceding it; first with the wires in the common position, and next when turned a quarter round, making the middle horary wire serve as a directing wire; for, if the wires do not cut each other at right angles, the difference of right ascension of the stars will come out too much one way, and as much too little the other way, and half the difference will be the correction in this case, whence it may be inferred in all other cases. Or, the angle of the deviation of the wires from a right angle being hence found, the correction of the difference of right ascension of the moon and star, is to the difference of their apparent declination; as the sine of the angle of the deviation of the wires is, to the cosine of the moon or star's declination.

I have determined the deviation of the wires of the telescope, which I used at St. Helena, by comparing them with a right angle, formed by two silver wires on a brass plate, fixed up in a window at the distance of 30 feet from the telescope. The extent of the compasses, with which the intersecting arches were struck, for finding the perpendicular lines

lines on the plate, being no less than seven inches, those wires may be supposed to differ insensibly from a right angle to each other. The telescope being adjusted for seeing them distinctly, I brought that wire of the telescope, which in celestial observations represented a parallel of declination, to be exactly parallel to one of the wires on the plate, with the smallest interval possible; and, at the same time, made the middle perpendicular, or horary wire, to pass through the intersection of both the wires in the window: when I plainly discerned, that the wires of the telescope were not exactly perpendicular to each other, the superior angle to the right being manifestly acute, and the superior one to the left obtuse. This I further verified by applying the acute angle to the left hand superior angle of the plate, turning the wires in the telescope a quarter round, from right to left, by the screw adapted for this purpose, when the same difference appeared as before. This proved also that the wires on the plate made exact right angles with each other; otherwise the acute angle of the wires of the telescope could not have appeared to differ equally from both of them. To find the exact difference of the angle made by the wires from a right angle, I had a third wire placed exactly parallel to one of the former on the plate at the distance of $\frac{1}{4}$ th of an inch; when by applying the angle of the wires of the telescope to the right angle on the plate, the deviation of the former from the latter appeared to be equal to half the interval of the parallel wires at the extremity of the field of view; but the semi-diameter of the field of the telescope at the distance of the wires in the window being

being measured $\frac{925}{1000}$ of an inch; whence the angle of deviation of the wires, from a right angle, is 21 minutes. But, by a mean of 11 trials, the quantity of the deviation came out $28' \frac{1}{2}$, the extreme results being 21' and 36'. This is the deviation of the south part of the middle horary wire, from a perpendicularity, to the directing wire towards the east, in the observations at St. Helena; a star, that passed south of the centre of the telescope, coming to the horary wire too soon, and a star that passed north of the centre coming later to the horary wire than it ought to do.

In order to determine whether the two other horary wires were parallel to the middle one, or, if not, what angle they made with it, I compared the transit of 13 stars across the three horary wires, with those of as many more stars differing considerably in declination from the former, all observed at St. Helena; and from the differences of right ascension at the several wires, after making an allowance for the convergence of the meridians, which however is not 2', I found the south part of the first or eastern wire to deviate from a parallelism with the middle one towards the west by $9',8$, and the western wire to deviate towards the east by $5',4$; hence it appears that the south parts of the eastern, middle, and western horary wire differed from a perpendicularity to the directing wire towards the east, in the observation of St. Helena, by $18',7$, $28' \frac{1}{2}$, and $34'$, the mean deviation of all the three being $27'$ or only $1' \frac{1}{2}$ different from that of the middle horary wire. This quantity of the mean deviation of the wires is also confirmed

firmed by a comparison of six differences of right ascension of stars observed at St. Helena, with the same observed, at my desire, since my return, at the Royal Observatory, by the transit instrument, which gives $27' \frac{1}{4}$, agreeing exactly with what has been here found in a more certain manner.

Sometimes, in making these observations, it so happened, that several stars lay near the moon's parallel of declination; when I observed all of them, that came within the field of the telescope; as well to obviate the hazard of missing to observe the right star again after an interval of several hours, as to obtain a greater number of comparisons of the moon's motions in right ascension, and so reduce the unavoidable errors of the observations as much as possible.

It may be proper to remark, that the most convenient time for making these observations, is when the moon is stationary at her greatest declinations; when she may be compared with the same star, with a telescope having a moderate field of view, for several hours. The change of the moon's parallax in declination is then alone to be feared; but if the observations are made nearly at the same distance from the meridian, both on the western and eastern side, the parallax returning to the same quantity, will occasion no difficulty. Sometimes, when the moon is not exactly at, but only near, her greatest declination, by observing her on the proper side of the meridian, the effect of parallax may be found to be contrary to, and consequently counteract, her change of declination arising from her proper motion.

Through

Through the great cloudiness of the skies at St. Helena, I could observe the moon only one night, namely January 8, 1762, near her limits of declination; on the other nights, I endeavoured to compare her with as many stars as came within the field of the telescope, trusting to determine afterwards the difference of right ascension of the stars, with which she was compared in the former and latter observations of the same night. This design I have since completed, by procuring a great many transits of these stars, to be observed on the meridian, at the Royal Observatory, in the latter end of the year 1762, and beginning of the year 1763; many also I made there myself. An account of them is given at the end of the other observations.

An useful remark here offers itself to our notice; that the moon's parallax may be very well determined, in a fixed observatory, at any period of her declination, by observing the difference of right ascension of her limb, and any star near her parallel, at a considerable distance from the meridian, either to the east or west, with the parallactic telescope, and also on the meridian with the transit instrument.

But, in pursuing this method, the parallactic telescope ought to be nearly of equal goodness with the transit telescope; else the moon's diameter might appear greater by some seconds through one and the other, and consequently the parallax so deduced would not be exact. It is true, that, by a proper method of comparing the observations, this small error might be obviated, though the telescopes differed ever so much in the degree of distinctness, namely, by taking a mean of the results found by

the observations in the first and last half of the moon, or on the eastern and western sides of the meridian; for it is manifest the errors would be of contrary tendency in these different cases.

As it may serve the more to recommend the practice of these observations to astronomers, I think it proper to mention, that I seldom failed of finding some proper star or stars near the moon, of sufficient brightness, to compare her with, even when there were none such marked down in any catalogue, or any charts: the number of zodiacal stars proper for comparing the moon and planets with in a telescope, and not inserted in any printed catalogue, seeming much to exceed the number of those marked down.

I have but one more remark to add on this subject, that, as it is necessary to know nearly the apparent difference of declination of the moon's centre, and the stars observed, in order to correct the observations for the deviation of the wires, and the moon's change of declination, so this may most readily and conveniently be done by some oblique wires fixed in the focus of the eye-glass of the telescope, as I had in that I used at Barbadoes. Not having such a contrivance adapted to the telescope I used at St. Helena, I always estimated by my eye how many minutes each star, and also the moon's centre, passed north or south of the directing wire, which I had an easy method of doing, by comparing their distance from the directing wire, with the interval between the directing wire, and one of the two wires stretched parallel to it, at the exact distance of 10 minutes on each side. In this manner the numbers
set

set down by the sides of most of the observations were deduced, expressing the number of minutes which the stars passed north or south of the moon's centre. They may be depended on to a minute, or two at most, which is sufficient for the reduction of the observations.

To make the foregoing account of the method of observing the moon's horary parallaxes more clear, let $ADBE$, [TAB. XXI.] represent the cell containing the silver wires stretched in the focus of the nearest eye-glass of the telescope, of which AB is the directing wire, MN , DE , OP , three wires perpendicular to AB representing portions of horary circles; and, FG , HI , two other wires parallel to the directing wire AB , at the equal distances, CQ , CR , on each side, equal to 10 minutes. The semi-diameter of the field of the telescope CA is 14 minutes; A is the eastern side of the field of view, B the western, D the southern, and E the northern side. In order to adjust the wire AB parallel to the equator, so that MN , DE , OP , may represent horary circles, the star, whose difference of right ascension from the moon is to be observed, is brought by means of the screws to the intersection of the wires AB , DE , at the centre of the telescope C , and when the star is near passing out of the telescope at B , the cell $ADBE$ is turned round, by means of a screw, till the star is again brought upon, and bisected by the wire AB ; this being done, if the screw be turned to make the telescope follow the star towards the west, and the star be again brought to the intersection of the wires at C , it will be generally found to run exactly along the wire CB , bisected by it all the way from C to B ; which is then a proof that the wire AB is rightly adjusted; but, if the star

runs

runs not exactly along the wire AB , the position of that wire must be altered a little, till the star runs exactly along it from the centre C , to the extremity of the field of the telescope at B . Then turn the screw to carry back the star from B a little to the east of the first or eastern wire MN , and observe the exact minute, second, and quarter of a second, of the stars passing the three horary wires MN , DE , OP ; the telescope remaining unmoved and undisturbed, observe, in like manner, the transit of the moon's enlightened limb across the same wires, whether she pass south of the star, as along the line ST , or north of the same, as along to the line UV ; and the observation is completed. The like observation being repeated, after an interval of several hours, we shall have the apparent motion of the moon in right ascension in this time; whence the moon's horary, and thence her horizontal parallax may be computed.

If the moon precedes the star, and the wire AB is adjusted by making some bright point in the moon run along it, and WX is supposed to be the true parallel of declination, it is manifest that the star will pass the horary wire at a , to the south of the centre of the telescope sooner, and to the north of the centre of the telescope later, than it passes the true horary circle YZ at e , by the time it takes to describe ae parallel to WX . Let CL be the apparent motion of the bright point of the moon, in four minutes of time; draw KL perpendicular to WX produced, and CK is the apparent motion reduced to a parallel of the equator in four minutes, and KL the apparent motion in declination in the same time; and, the

the right-angled triangles aeC , KCL being similar, ae is to eC , as KL to CK ; but the error of the right ascension answering to ae is to ae , as radius to the cosine of the star's declination; and CK , is to the moon's apparent angular motion about the pole, in four minutes, as cosine of moon's apparent declination, to radius. Whence, by composition of ratio's, and by equality, the correction of the moon's right ascension is to eC the apparent difference of declination of the moon and star, in a compound ratio of the moon's apparent motion, in declination in four minutes, to her apparent motion about the pole in the same time, and of the square of the radius, to the product of the cosines of the stars and moon's apparent declinations.

Further it appears from the scheme, that the moon comes later to C than to the horary circle passing through the point of the wire (a) cut by a star between C and D , whose parallel of declination she is approaching, and that she comes sooner to C than to the horary circle passing through the point of the wire (a) cut by a star between C and E , whose parallel of declination she is receding from, by the time the star takes to describe ae ; and, therefore, the right ascension of the moon deduced immediately from that of the star must be too great in the first case, and too little in the second case, by the space ae measured upon the star's parallel of declination.

Lastly, to explain the manner of examining the deviation of the wires, from a perpendicular to each other, by observations of the stars; let AB , fig. 2. represent the directing, and ED the middle horary wire, deviating from CE , supposed perpendicular to

to AB , by the small angle DCF . Let any star be made to run along the wire AB , from A to B , any other star following it will pass the wire DE , at K sooner, than the horary circle FG at L , by the time of its describing the small space LK , and consequently the difference of right ascension will appear too little; now let the wires be turned a quarter round, that the wires AB , DE , may change places, D coming into the place of A , and E into that of B , which is done by making the first star run along the wire DE , from C to E . Now the wire AB , deviating from CI perpendicular to DE , by the angle BCI , the second star will pass the wire BC at N , later than the horary circle IC at M , by the time it takes to describe the space $MN = LK$; and consequently the difference of right ascension of the two stars will appear as much too great, as it before appeared too little, when the wires were adjusted in their usual position: and half the difference will be the correction in this case, to be added to the first, or subtracted from the second difference of right ascension; whence the correction may be easily inferred for all other observations.

CELESTIAL OBSERVATIONS.

Eclipse of the Moon, May 18, 1761.

Appt Time.

H , "

7 48 38	Penumbra plainly entered upon the Moon's disc.
7 56 23	Beginning of the Eclipse.
7 19 16	Shadow begins to touch Kepler.
7 37 8	Shadow bisects Manilius.
7 40 8	Shadow begins to touch Julius Cæsar.
10 39 23	Emerfion out of total darkness.
11 46 52	End of the Eclipse.

Immersion and Emerfions of Jupiter's Satellites, observed at the Observatory on the Alarum Hill.

1761	Appt Time	
Day of the Month.	H , "	
17 July 20	17 44 31	Imm. 1 Sat. The Moon near Jupiter.
18 July 22	12 12 29	Imm. 1 Sat.
18 Aug. 5	14 49 50	3 Sat. almost immerg'd. Then clouds.
	14 50 30	3 Sat. certainly immerg'd.
	16 0 40	Imm. 1 Sat.
24 Aug. 27	11 13 56	Emerf. 4 Sat.

D

Im-

Immersion and Emergences of Jupiter's Satellites, observed in James's Valley.

1761.			Apparent Time.			
Day of the Month.			H	'	"	
h	Oct.	10	12	48	38	Em. 2 Sat.
24	Oct.	15	13	36	10 :	Em. 1 Sat.
9.	Oct.	16 {	10	3	40	Em. 3 Sat.
			10	3	45	Ditto by Mr. Mason (who arrived here to-day from the Cape of Good Hope) with a two foot reflecting telescope.
						Em. 1 Sat. Instantaneous both to myself and Mr. Mason, exactly at the same second, in different houses. Air very clear, and Satellite increased its light very fast.
1	Nov.	9	8	19	54	Em. 2 Sat. by Mr. Mason. I missed the instant of Emergion by moving the stand of my telescope. Air very clear.
8	Nov.	11	12	40	19	Em. 1 Sat.
1	Nov.	23	12	9	52	Imm. 3 Sat. Air a little hazy.
			7	39	18	Ditto by Mr. Mason.
1	Nov.	28 {	7	39	13	3 Sat. had not em. Then clouds.
			10	8	17	Ditto plainly out, though not near arriv'd to its full lustre.
			10	10	13	2 Sat. had emerg'd. Its light was so weak, it probably had not been out above 15 seconds.
⊙	Nov.	29	7	9	48	Em. 2 Sat.
⊙	Dec.	6 {	9	43	36	Ditto by Mr. Mason.
			9	43	33	
1762.						
⊙	Jan.	10	7	37	42	Imm. 3 Sat.

N. B. This mark (⊙) affixed to any observation signifies that it is a little uncertain.

Observations

Observations of the difference of right ascension, by time, between the moon's enlightened limb and stars, taken by the help of the parallaetic wires adapted to the reflecting telescope; designed for determining the horary parallaxes of the moon. They may also serve to deduce the longitude of the place.

24 Sept. 3. Compared the moon's western and preceding limb, with α Libræ in right ascension, at the Observatory on the Alarum Hill. The times are by the clock, which is 12 seconds too slow for sidereal time, and keep the rate of going of sidereal time, exactly. The star passed south of the moon's centre.

		1st hor. w.	2d hor. w.	3d hor. w.	D's Limb at middle wire.
	H				Apparent Time.
					H
α Libræ	17	42. 9	42 23	44 37	
D's W. Limb	17	43 45	43 59 $\frac{1}{2}$	44 13 $\frac{1}{2}$	6 52 49
α Libræ	18	2 10	2 24	2 38	
D's W. Limb	18	4 14	4 28 $\frac{1}{2}$	4 42 $\frac{1}{2}$	7 13 15
α Libræ	18	16 3	16 17	16 31	
D's W. Limb	18	18 27 $\frac{1}{2}$	18 42	18 56	7 27 26
α Libræ	18	23 6	23 20	23 34	
D's W. Limb	18	25 41	25 55	26 9+	7 34 38

Thursday September 24, I removed the clock down to James's Valley, and keeping the same length of the pendulum as before, fixed it up strongly against the wall of a house, in an upper room, whence I could make my observations through openings made in the roof of the house. I fixed the equal altitude instrument, for regulating the clock, against a strong post, let deep into the ground, in a little room eight foot square, built for this purpose, in a convenient open place, at a little distance from

the house where the clock was. When I observed the sun's equal altitudes, I first adjusted the instrument; then I went to the room where the clock was, and set my watch, having a second hand, exactly with it; then I returned to the equal altitude instrument, and observed the passage of the sun's limb across the horizontal wires of the instrument, according to the time shewn by the watch; and, immediately after the observation, went again to the clock, and compared the watch with it, noting how much it had got or lost, whence the observations were easily reduced to the time of the clock.

		1 Wire	2 Wire	3 Wire	Apparent Time	
	H					H
4 Oct. 8	D's W. L.	11	34	2	34	17
	Capricorni	11	Clouds	36	30	Clouds
					34	31
					10	45
						14
						Star 10' N. of M.
2 Oct. 10	D's W. L.	14	19	57	20	12
	Aquarii	14	24	57	25	11
					25	24

October 8th and 10th the clock got 7", 3 upon 24 hours in one revolution of the fixed stars.

The foregoing observations were all made with the telescope placed upon the common stand, without the polar axis. The observations of December 4th, that follow, were made with the telescope fixed upon a new and heavier stand, which was rendered more steady, by two broad feet resting upon several of the boards at once. The socket for receiving the telescope was cut obliquely in the stand, so that it had partly the effect of a polar axis.

October 28th, I took down the clock, packed it up, and sent it on board a vessel going to the Cape of Good Hope, to return again soon, committing it to the care of Mr. Jeremiah Dixon, who had observed the transit of Venus over the sun at the Cape. He took his passage on board the said vessel, in order to set the clock up at the Cape of Good Hope, and examine the difference of its going between that place and St. Helena, for determining the proportion of the force of gravity at those two places.

The same day Mr. Mason fixed his clock up, for my use, against a large masonry post, let deep into the ground, near the equal altitude instrument, at the little Observatory. This clock was made by Mr. John Ellicott, F. R. S.

I still continued, for some time, to make my observations in the upper room, as before. For this purpose I fixed up a little clock there, which may be called a journeyman, or secondary clock, having a pendulum swinging seconds, which, after being well adjusted, would keep time very regularly for several hours. It had only a minute and second hands, and struck every minute exactly as the second hand came to sixty, which, was very convenient for the counting of seconds; more especially in the observations made with the parallactic telescope, it being improper, on account of the instability of the floor, to get up from one's seat, or to alter the position of the body considerably even to catch the second, till those observations were completed. I reduced the times to that of the observatory clock, by means of my watch, with the second hand. The little clock, as well as the larger clock, which I sent with Mr. Dixon to the Cape of Good Hope, was made by Mr. John Shelton.

♀ December 4th, I compared the moon's western limb with the three ♀ of Aquarius, with respect to right ascension, and observed her occult to the southern one. The time is set down according to the little clock, and the difference between that and the observatory clock is set down by the side; the latter lost 59 seconds upon 24 hours, in one revolution of the fixed stars, and the little clock kept very nearly the rate of sidereal time. In these observations, the directing wire was adjusted by the stars.

Wire

[30]

	1 Wire	2 Wire	3 Wire	Lit. clock flow. than obs. clock	D's L. at mid. wire by observa- tory clock.	Appt Time
	H , "	" , "	" , "	" , "	H , "	H , "
D's W. Limb	23 21 0	21 13 $\frac{1}{2}$	21 27	8. 4	23 29 17 $\frac{1}{2}$	7 19 5
3d & Aquarii	23 23 32	23 46	23 59			
D's W. Limb	23 38 31	38 45	38 59	8. 4	23 46 49	7 36 34
3d & Aquar.	23 40 46	40 59 $\frac{1}{2}$	41 13			
D's W. Limb	0 47 17	47 31	47 44	8. c	0 55 31	8 45 6
3d & Aquar.	0 48 18	48 32	48 45 $\frac{1}{2}$			
D's W. Limb	0 58 0	58 15	58 28	8. c	1. 6. 15	8 55 48
3d & Aquar.	1 58 50	59 3 $\frac{1}{2}$	59 17			
2d & Aquar.	1 4 10	4 24	4 37	8. c		
D's W. Limb	1 4 30	Clouds	4 58+		1 12 44	9 2 7
D's W. Limb	1 17 33	17 47	18 1	7 59	1 25 46	9 15 17
3d & Aquar.	Clouds	18 13 $\frac{1}{2}$	Clouds			
2d & Aquar.	1 25 7 $\frac{1}{2}$	25 21 $\frac{1}{2}$	25 35			
D's W. Limb	1 25 54	26 8	26 22	7 59	1 34 7	9 23 37
1st & Aquar.	1 31 27	31 40	31 54			
2d & Aquar.	1 33 33	33 47	33 0			
D's W. Limb	1 34 30	34 44	34 58	7 58	1 42 42	9 32 10
1st & Aquar.	1 45 31 $\frac{1}{2}$	45 45 $\frac{1}{2}$	45 59 $\frac{1}{2}$			
2d & Aquar.	1 47 37	47 52	+8 5+			
D's W. Limb	1 48 53 $\frac{1}{2}$	49 7 $\frac{1}{2}$	49 21	7 58	1 57 5	9 46 31
1st & Aquar.	1 58 57	59 11	59 24 $\frac{1}{2}$			
D's W. Limb	2 2 37 $\frac{1}{2}$	2 52	3 5 $\frac{1}{2}$	7 57	2 10 49	10 0 13
1st & Aquar.	2 9 16	9 30	9 43			
D's W. Limb	2 13 11	13 25	13 39	7 57	2 21 22	11 10 45
1st & Aquar.	2 20 6+	20 20	20 33 $\frac{1}{2}$			
2d & Aquar.	Clouds	22 26	Clouds			
D's W. Limb	2 24 17	24 31 $\frac{1}{2}$	24 45	7 56	2 32 27	10 21 48

At

At 23^h. 49^m. 48^s. by little clock, or 23^h. 57^m. 51^s. by observatory clock, which is 7^h. 47^m. 34^s. apparent time, the 3d γ of Aquarius vanished instantaneously, clouds coming over the moon at the same time. Therefore it remains a little dubious, whether this was the very instant of the star's occultation by the moon, or whether it was obscured by the clouds, though I rather suppose the former from the manner of its vanishing, and also because when the clouds cleared away presently the star was gone.

December 4th, by equal altitudes, the sun passed the meridian at 16 H. 9 M. 11,3 S. and December 5th at 16 H. 12 M. 33,4 S. by the observatory clock, whence the observed times are easily reduced to apparent time, as above.

Finding the above observations of December 4th (though they may be depended on to half a second of time) to be still incommoded by a small trembling of the telescope, owing to its resting on a floor; I determined, for the future, to make these observations, at the little observatory, on the ground, which I caused to be altered, to make it more convenient for this purpose. Here I constantly made use of the polar axis, which I found to afford considerable advantages with respect to the facility and exactness of making the observations.

♀ January 8th 1762. Compared the moon's western limb, with several stars, with respect to right ascension, at the little observatory. The four stars, with which the moon was compared, are distinguished by letters, according to the order of their right ascension.

	1st Wire	2d Wire	3d Wire	Stars N. or S. of D's centre	D's Limb at Mid. Wire
	H " "	" " "	" " "		Apparent Time H " "
a	2 48 30—	48 45	49 0	13 $\frac{1}{2}$ N.	
d	Clouds	55 53+	55 48+	4 $\frac{1}{2}$ N.	
e	5 56 10+	56 25+	Clouds	15 $\frac{1}{2}$ N.	
D	2 57 3	57 18	Clouds		8 41 58
a	3 16 32	17	17 22+	14 $\frac{1}{2}$ N.	
b	3 19 12—	19 27	19 42+	22 N.	
c	3 22 38	22 53+	Clouds	13 $\frac{1}{2}$ N.	
D	3 26 16	26 32	26 47 $\frac{1}{2}$		9 11 8
a	4 22 38—	22 53—	23 8—	12 N.	
b	4 24 57	25 13—	25 27 $\frac{1}{2}$	19 N.	
c	4 Clouds	28 39	28 54	11 $\frac{1}{2}$ N.	
e	4 30 18	Clouds	30 48—	12 $\frac{1}{2}$ N.	
D	4 33 55	34 11 $\frac{1}{2}$	34 27		10 18 38
a	4 52 44—	52 59	53 14		
b	4 Clouds	55 19	55 34	b 7 N. of a	
D	5 4 54:	5 9 $\frac{1}{2}$	Clouds		10 49 31 $\frac{1}{2}$
e	5 15 9	Clouds	15 39 $\frac{1}{2}$		
D	5 20 4 $\frac{1}{2}$	20 20	20 35 $\frac{1}{2}$		11 4 40
*	5 30 52	31 7 $\frac{1}{2}$	31 22 $\frac{1}{2}$		
a	5 33 15	33 30	33 45	20 circ. of *	
a	5 57 39	57 54+	58 9+	6 $\frac{1}{2}$ N.	
b	5 59 59+	0 14 $\frac{1}{2}$		13 $\frac{1}{2}$ N.	
c	6 3 25 $\frac{1}{2}$	3 40 $\frac{1}{2}$	3 55 $\frac{1}{2}$	6 $\frac{1}{2}$ N.	
e	6 5 20	5 35	5 50	7 $\frac{1}{2}$ N.	
D	6 11 44	12 0—	12 15		11 56 12
*	6 18 18 $\frac{1}{2}$	18 34	18 49		
a	6 20 43	20 58	21 13	20 cir. of *	
b	6 23 2	Clouds	23 32 $\frac{1}{2}$	13 $\frac{1}{2}$ S. of ditto	
c	6 26 28+	26 43 $\frac{1}{2}$	26 59	21 $\frac{1}{2}$ S. of ditto	
e	6 28 23	28 38+	28 53—	19 $\frac{1}{2}$ S. of ditto	
e	7 55 56	56 11 $\frac{1}{2}$	56 26+	10 S.	
D	8 5 56	6 27	6 27		13 50 9 1/2 January

January 9. Compared D's western limb with three stars.
Adjusted the directing wire by a bright point in the moon.

	1st Wire	2d Wire	3d Wire	Stars N. or S. of D's centre	D's Limb at Mid. Wire
	H / "	/ "	/ "	/	Apparent Time H
D L.	3 59 42	59 58	0 13 $\frac{1}{2}$		9 41 8
1 *	4	17 cir.		10 cir. S.	
2 *	4	19 42		1 N.	
3 *	4 23 10	23 26	23 40 $\frac{1}{2}$	11 $\frac{1}{2}$ S.	

January 9. from equal altitudes, the sun passed the meridian at 18 H. 17 M. 29 S. and January 10, at 18 H. 20 M. 50.6 S. per clock, which loses 59 S. upon the rate of sidereal time in one revolution of the fixed stars: therefore the sun may be computed to have past the meridian, January 8, at 18 H. 14 M. 7 S.

☿	February 3				
a	4 49 31	49 45 $\frac{1}{2}$	50 0		
b	4 50	17	32	The minute not noted, but from following observations must have been 50 M. to middle wire.	
D	4 53 23 $\frac{1}{2}$	53 59	53 54		
					7 44 28
b	5 0 6 $\frac{1}{2}$	0 21	0 36		
D	5 3 42	3 57	4 12		7 54 44

♀	February 5				
D	5 0 0 $\frac{1}{2}$	0 16	0 31 $\frac{1}{2}$		7 42 52
*	5		10 13	6 N.	
D	5 44 12 $\frac{1}{2}$	44 28 $\frac{1}{2}$	44 44		8 26 56
*	5 52 40	52 55	53 10	5 N.	
D	5 57 7	57 23	57 38		8 39 48
*	6 5 12 $\frac{1}{2}$	5 27 $\frac{1}{2}$	5 43		
D	6 22 24	22 39	22 55		9 5 0
*	6	30 3	30 18		

E

1st Wire

	1st Wire	2d Wire	3d Wire	Stars N. or S. of D's centre	D's Limb at Mid. Wire
H	"	"	"	"	Apparent Time H
☉	February 7.				
D	6 36 5	36 20	36 35		9 10 29
	6	51 ½ cir.			
g	6 58 48		59 17½	7 N.	
i	6 59 49+	0. 4		12 N.	
l	7 1 24			12 S.	
D	7 24 4	24 19	24 34		9 58 20
	7	39 cir.		13 S.	
l	7 48 2	48 17	48 32	3 ½ S.	
q	8 2 7	2 22	2 36+	5 ¾ S.	
D	9 7 10	7 25	7 40		11 41 8
a	9		16 1+	10 S.	
b	9		18 20		
c			18 32		
b	9 26 3	26 17½	26 32	7 ½ S.	
k	9		27 46		
l	9	28 38	28 52½	8 ⅞ N.	
m	9	32 15			
n	9 32 18	32 32	32 46½		
o	9 36 42	36 57+	37 11+	6 S.	
p	9 38 29	38 43½	38 58+	6 N.	
q	9 42 27+	42 42	42 56½	5 ¾ N.	
D	10 9 38½	9 53½	10 9		12 43 26
c	10 18 51	19 6	19 20+	14 N.	
f	10	26 32		0	
k	10	28 21			
l	10	29 27		22 N.	
q	10 43 16	43 31	43 46	18 ⅔ N.	
D	10 51 34½	51 49½	52 4+		13 25 15
a	10 57 8	57 23		13 ⅓ N.	
f	11	7 17+	7 32	9 ½ N.	

1st Wire

	1 Wire	2 Wire	3 Wire	Stars N. or S. of D's centre	D's Limb at Mid. Wire
	H . "	. "	. "		Apparent Time H . "
D	12 22 10	22 25+	22 40+		14 55 36
d	12 29 16	29 31—	29 45+	12 $\frac{1}{10}$ S.	
D	12 36 17+	36 32 $\frac{1}{2}$	36 48—		15 9 44
d	12 42 54 $\frac{1}{2}$	43 9	43 24—	9 $\frac{1}{10}$ S.	
e	12	44 9—	44 23 $\frac{1}{2}$	12 S.	
D	12 53 10	53 26—	53 40+		15 26 31
d	12 59 13—	59 27	59 41 $\frac{1}{2}$		
e	13 0 12	0 27—	0 41		
D	13 7 27	7 42	7 57		15 40 45
d	13 12 58 $\frac{1}{2}$	13 13	13 28		
D	13 21 32	21 48—	22 3		15 54 48
e	13	27 48+		3 S.	

δ	February 9				
D	3 59 42	59 58	0 13 $\frac{1}{2}$		6 26 26
	4	17 cir.		11 S.	
	4	19 42		1 N.	
	4 23 10	23 26—	23 40 $\frac{1}{2}$	11 S.	

The observations from February 3^d were made with my own clock, with which Mr. Dixon returned from the Cape of Good Hope December 30th, 1761, after examining the going of it there. He found it to get there 36,6 seconds upon sidereal time in one revolution of the fixed stars, or 29.3 seconds *per* day more than it got at St. Helena with the same length of pendulum: but I propose to give a more particular account of these, and some other experiments then made by Mr. Dixon at the Cape, some other opportunity.

From equal altitudes, the sun passed the meridian, January 30th, at 20^h 51' 8" $\frac{3}{4}$; February 5th, at 21^h 16' 5", 6; and February 7th, at 21^h 24' 17", 7. Hence the clock appears to

have got at the rate of $6''.3$ upon sidereal time in 24 hours. By the setting of four stars behind the hill, observed with the telescope of the equal altitude instrument, January 29th, and again February 7th and 8th (after the manner described by Mr. Mason in his account of the going of Mr. Ellicott's clock determined by him and myself in this manner, Phil. Trans. Vol. LII. Part II. Page 534.) the clock appeared to get $6''.25$ upon sidereal time in one revolution of the stars, which agrees exactly with the former determination by the sun's equal altitudes. In like manner, I always found the going of the clock, determined by these two different methods, would come out as nearly the same as the equal altitudes of the sun could be depended upon, that is to say, to a second, even from the observations of two successive days.

I must not pass by this occasion, without taking notice of some remarks, which Mr. Short passes on my method of examining the going of the clock, by observing stars setting behind a hill, with the telescope of the equal altitude instrument; (vide Mr. Short's account of Mr. Mason's paper concerning the going of Mr. Ellicott's clock at St. Helena. Phil. Trans. Vol. LII. Part II. Page 540). Mr. Short represents Mr. Mason, as saying in his paper, that I proposed making use of the equal altitude instrument to determine the regularity of the motion of Mr. Ellicott's clock, by observing the vanishing of the stars out of the field of the telescope, an expression not contained in Mr. Mason's paper, who is only speaking of our observing stars setting behind a hill, at the distance of a quarter of a mile, in the same part of the field of the equal altitude instrument. Had we proceeded in the method supposed in the remarks, no doubt the observations would have been liable to considerable inaccuracy: but as we used the telescope of the equal altitude instrument, only to assist the sight in observing the stars setting behind the hill, we were liable to no other error than what might arise from the small alterations of the instrument, arising from the changes of heat and cold, moisture and driness, seen from the distance of the top of the hill, which will easily be allowed to be quite insensible. And, indeed, how otherwise could the observations, contained in Mr. Mason's paper, agree so well together as they do? A circumstance alone sufficient to create a suspicion of the objection being ill grounded. The reason of Mr. Mason and myself always observing the stars to vanish behind the hill, in the same part of the field of the telescope (that is,

is, very near its centre) was, in order to keep the object glass at the same height; though this being less than an inch in diameter, and consequently subtending less than 13" from the top of the hill, there could not have been a second of time difference, whether the stars had been observed to vanish behind the hill, either in the upper or lower part of the field of view.

Mr. Short also remarks, that no inference can be formed with respect to the different forces of gravity, in different latitudes, from experiments made with clocks, because the same clock, set up on different sides of the same room, will be found to differ considerably from itself. I readily allow that, if clocks are fixed up in a slight manner, or against common wainscots, the experiments made with them cannot be depended upon. Nevertheless it does not appear, but that when they are fixed in a firmer manner, they may be depended upon near enough to be of a considerable use in physical enquiries: which I have reason to think from the many experiments I have tried with the Royal Society's clock, made by Mr. John Shelton, which I propose to give a particular account of at some other opportunity.

Observations of the Sun's setting in the Sea.

At the Observatory at the Alarum-House, which, by careful mensuration, I found to be elevated 1983 feet above the level of the sea. Therefore the height of the eye is 1988 feet.

1761	App ^t Time			
	H	'	"	
June 14	5	39	5 $\frac{1}{2}$	☉'s upper limb set in the sea.
June 16	5	39	1	☉'s upper limb set. A little cloudy.
July 18	5	39	34	☉'s L. L. set in the sea, certain to 2 or 3 S.
	5	42	8	☉'s U. L. set in the sea, certain to $\frac{1}{2}$ S.
July 30	5	44	21	☉'s U. L. set in the sea. A little cloudy.

The

The like Observations made in James's-Valley, near the Sea-side.

1761	App ^t Time	H	'	"		
Nov. 16	{	6	23	42	☉'s L. L. set	} Height of eye above the sea 16 feet.
		6	25	22	☉'s centre set	
		6	26	21	☉'s U. L. set	
Dec. 15	{	6	29	24	☉'s L. L. set	} Height of eye above the sea 15 feet.
		6	32	10	☉'s U. L. set	
Dec. 16		6	32	11½	{ ☉'s U. L. set, thro' a small apperture in a cloud. The height of eye above the sea 15 feet.	
Dec. 17	{	6	29	24	☉'s L. L. set	} Height of eye above the sea 15 feet.
		6	30	52½	☉'s centre set	
		6	32	20½	☉'s U. L. set	

♄ August 5th

App^t Time

H ' "

5. 6. 7 { A star 6th magn. in Pisces, at centre of equal altitude instrument.

5. 6. 32 Jupiter's centre at the horizontal wire.

5 7 8 Jupiter's centre at the vertical wire.

The telescope remained fixed during these observations.

Again.

17 31 30 The star at the vertical wire.

17 31 46 The star at the horizontal wire.

17 32 11 Jupiter's centre at the horizontal wire.

17 32 37 Jupiter's centre at the vertical wire.

Here also the telescope remained fixed during the observations.

♄ August 19th. Found the little star, which is 14" N. of ♄ Scorpii, to precede it one second of time, by my parallactic wires

wires, with my watch, which makes four beats in a second of time. If any thing the difference was something more than a second of time: the little star may therefore be supposed to precede β Scorpii $17''$ in right ascension. By observations made with the 10 foot sector on several nights, while β Scorpii and the little star were passing the field of the telescope, I found the little star to be exactly $14''$ N. of β Scorpii in declination. For June 24 the difference was $12'',5$. July 21, $13'',6$. July 22, $14'',7$. July 23, $15'',3$: August 14, $12'',9$. August 17, $14'',8$.

The bright star in the foot of the Centaur, marked α in the catalogues, when viewed through a telescope, becomes divided into two stars, one of which is about the second, and the other about the fourth magnitude. They were both observed by the Abbé de la Caille. I found their distance, by the divided object glass micrometer fitted to the reflecting telescope, to be $15''$ or $16''$. But it is, in a manner, impossible to measure the distance of two stars very accurately with this micrometer, for being similar lucid objects, when they are brought very near each other, their light will be confounded together before they exactly coincide.

The larger Magellanic cloud, viewed through a telescope, exhibits a few stars which then appear separated to a considerable distance from each other. Their being so few in number, and so thinly scattered, is the reason of this phenomenon appearing so very faint.

The less Magellanic cloud, viewed through a telescope, exhibits a very remarkable lucid nebula, with some tolerable bright stars surrounding it. The nebula appears nearly circular, about $3'$ in diameter.

Transits of stars, with which the moon was compared, February 7, 1762, at St. Helena, taken upon the meridian, at the Royal Observatory, in the beginning of the year 1763, for ascertaining the exact difference of their right ascension. Vide page 19.

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire	
		' "	' "	H ' "	' "	' "	
January 16	a		52 52	8 53 31	54 21		
	c	54 41	55 20+	8 55 59+			
	d		57 17	57 56	58 35	59 14	ξ of Cancer
	e	57 38	58 16½	58 56	59 35	0 13½	
	b	2 42::	3 21::	9 4 1:			
	f					4 44	
	l		5 40+	6 20	6 59	7 38½	
	o			9 13 39:			
	q	19 5	19 44+	9 20 23½	21 3	21 14½	λ of the Lion

Note, the clock gets five seconds per day upon fidereal time.

March 6	c		52 39	8 53 18	53 57	54 36	
	b			9 1 19	1 58½		
	i				2 42		
	l				4 18	4 57	
	m				7 34	8 14	
	q			17 43—	18 22	19 1	

Clock loses one second and an half per day.

March 7	a		50 10	8 50 49+	51 29		
	c	51 59	51 37+	53 17	53 56+		
	d		54 34½	55 13½	55 52½	56 31	
	e		55 33½	56 14	56 52½	57 31	
	b			9 1 17½			
	l		2 58	3 37	4 17	4 55½	
	q		17 2	17 41	18 21	19 0	

March 8	a			8	51 27		
	c	51 57	52 37	53 15			
	d	53 54	54 32½	55 12	55 51	56 29½	
	e	54 54	55 33	56 12	56 51	57 29½	
	i	0 41		9 2 0	2 39	3 18	
	l				4 15+	4 53	
	q	16 21	17 0	17 39½	18 19	18 57½	

1 Wire

[41]

		1 Wire	2 Wire	3 Wire	4 Wire	5 Wire
		" "	" "	H " "	" "	" "
March 9	a	49 28	50 7	8 50 45 $\frac{1}{2}$	51 25	
	c	51 55+	52 34	53 13—		
	d	53 52 $\frac{1}{2}$	54 31	55 10	55 49	56 27
	e	54 52 $\frac{1}{2}$	55 31	56 10	56 49	57 28
	f			9 0 41		
	h			1 14+	1 55:	
	l		2 54 $\frac{1}{2}$	3 34	4 13 $\frac{1}{2}$	4 52
	n			7 30		
	o			10 54:	11 34:	
	q	16 20—	16 59	17 38	18 17+	18 56
March 12	a			8	51 52	52 31
	c	52 23	53 2	8 53 41	54 20 $\frac{1}{2}$	
	d		54 59	55 38	56 16 $\frac{1}{2}$	
	e		55 59	56 39	57 17	57 55
	g			9	2 5	
	l					5 19
	q		17 26—	9 18 6	18 45	19 24
March 13	a		50 33—	8 51 12	51 51+	52 30 $\frac{1}{2}$
	c	52 22	53 0..	53 40		
	d	54 19	54 57+	55 36 $\frac{1}{2}$	56 15 $\frac{1}{2}$	
	e	55 19	55 57	56 37 $\frac{1}{2}$	57 15+	57 54
March 15	a			8 51 11	51 50	52 29
	c	52 21	53 0+	53 39		
	d		55 57	56 36		
	e		56 57	57 36 $\frac{1}{2}$		
	q	16 46	17 25	9 18 4+	18 44	19 22+

Clock loses $\frac{1}{8}$ second per day.

April 6	a			8 50 54	51 33	
	c	52 5	52 43	53 22		
	d	54 1	54 40	55 19	55 58	
	e	55 1	55 40	56 19	56 58	
	g		59 25	9 1 7		
	i	0 48 $\frac{1}{2}$::	1 27	2 7		
	q	16 28	17 7+	9 17 47	18 26	19 5 $\frac{1}{2}$

Clock loses $\frac{3}{4}$ second per day.

N. B. By a mean of several transits of stars observed about this time, as well as by the foregoing observations, it appears that the intervals of the four first wires are exactly equal, and that the interval of the two last wires is smaller than the others by $\frac{1}{8}$ th part, which answers to $\frac{4}{16}$ th of a second of time in the transits of the above stars.

F

By

By several tranfits over the meridian, observed at the Royal Observatory, at the latter end of the year 1762 and beginning of 1763, the first or the preceding star of the three γ 's of Aquarius (with all which I compared the moon, December 4, 1761, at St. Helena) preceded the second or subsequent one $2'.7'',11$ of time in right ascension, and the third or last $3'.9'',70$ and the second preceded the third $1'.2'',59$ all according to the time of a clock regulated to agree with the diurnal revolution of the stars.

THE END.
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